THURSDAY, SEPTEMBER 8, 1887.

HYDROPHOBIA.

THE culmination of scientific knowledge in any special direction frequently appears to the casual observer as a sudden and unforeseen event, although actually the result of a combination of well-ascertained facts accumulated during many years. The introduction of rabies from among the inexact to among the more exactly known diseases has been however so rapid as to very fairly substantiate this popular belief. It is now scarcely more than three years ago since the self-sacrificing labours of M. Pasteur helped us to pass from superstition to accurate knowledge of the real nature of rabies or hydrophobia, and this passage from the pre-scientific to the scientific epoch of the subject was actually perfectly abrupt. Nearly the same thing may also be said of the discovery by Koch of the *Bacillus tuberculosis*, for in both cases the scientific grasp of the subject undoubtedly commenced with the discovery how the virus might be isolated for purposes of experiment. While, however, this is strictly the case with tuberculosis, it is but partially true for rabies, for though there cannot be a shadow of doubt that the micro-organism which is the virus of rabies will soon be demonstrated in pure cultivations, this one factor is yet wanting to place it on the same level as that of tuberculosis. This difficulty in obtaining pure cultures, though a serious defect in our information, is yet most interesting, for it affords a distinct interpretation of several facts in the etiology of the disease, which, as we shall see directly, proved obstacles to early inquirers, and which yet at the same time, when viewed in the present light of science, are most encouraging to those who are anxious to see this miserable evil extinguished for ever.

All observers, notably Mr. Dowdeswell, are agreed that a micrococcus can be demonstrated in the tissues of the spinal cord of animals affected with the disease, but unless we accept the doubtful results of Fol no one has yet succeeded in cultivating this micrococcus. Those familiar with the difficulties of "rearing" pathogenic organisms will readily understand this obstacle in a disease with such a long incubation period as rabies.

M. Pasteur at the outset of his investigations attempted to solve the problem in this direction, but fortunately for science soon abandoned it in view of the probability that the virus would best be dealt with by endeavouring to obtain it in quantity from the central nervous system, since from the symptoms it evidently there produced its greatest effect, and so might be expected to be more especially present. He therefore made an emulsion by crushing in sterilized *bouillon* portions of the central nervous system, specially the spinal cord. With this emulsion he inoculated the disease from animal to animal by injecting a small quantity of it beneath the dura mater. By this simple procedure he established the first of his most important discoveries, viz. the real incubation period of the disease. At the same time too, as is usual in instances of a genuine scientific advance, the one important discovery led to a further one, since he has thus presented science with an infallible means of determining whether an animal had really suffered from the disease or not.

To the public this test has already been of the utmost value, since, as is well known, the characteristic lesions in the alimentary canal, &c., being absent or but feebly marked in the early stages of the disease, the possibility of thus giving a definite opinion in cases of doubt by the aid of Pasteur's method has frequently been the means of affording the utmost relief to the minds of those who have been victims to the lingering dread of hydrophobia. We are not aware however that the slightest public expression of gratitude has ever been expressed in this country towards that experimental science which in M. Pasteur's hands has led to such an important result, or that we, who by reason of rabies being endemic among us are profiting, and will profit, enormously by the light thus shed on the subject, have acknowledged our indebtedness to him in any way.

From his researches M. Pasteur was led on to formulate certain deductions which might be accepted as the logical consequence of the theory of the disease thus shown by him to be indubitably zymotic.

In the first place, M. Pasteur considered it probable that he could attenuate the virus which he had just discovered the possibility of handling with certainty, and we may add safety. This he soon accomplished by the method of drying.

It might naturally be supposed that, having proceeded thus far, he would have been led to attempt protection from, and prevention of, the evil effects of the disease by inoculating with this attenuated virus. Indeed it is unfortunately the popular belief that he did do this, and that his efforts to cure the malady are conducted upon this plan.

As a matter of fact, however, M. Pasteur, with a much wider prescience of the facts and theories of zymotic disease in general, considered that the well-known arrest of development which happens to virulent organisms as a consequence of their growth in tissues or in artificial culture media was due to the production by their metabolic processes of katalyzed substances whose presence was inimical to the active growth of the microbe, and that therefore these substances might be regarded and used as antidotes to the virus. Acting on this assumption, he proceeded to endeavour to protect animals by injecting into them considerable quantities, not of the virus, as generally supposed, but of these antidotal substances, or possibly of one alone. Of course, knowing that as a rule attempts to isolate in a state of chemical purity such products usually failed or seriously altered them, and having already ascertained that the baleful influence of the virus could be abrogated or rendered nugatory by the process of drying, he proceeded to employ the simple method of injection emulsions of dried spinal cords. The injection of such emulsions was naturally performed understanding that the drying would probably also impair to a certain extent the protective value of the antidotal substance; to insure, therefore, the introduction into the animal of as large a quantity as possible he considered it advisable to inject emulsions on successive days from spinal cords which had been dried for shorter and shorter periods, till ultimately, having commenced by inoculating from cords which had been dried for fourteen days, and which had been proved to be perfectly innocuous, he arrived at a cord which had been dried for only twenty-four hours. Of course the introduction of such virulent matter as this had no effect on systems already armed by the previous inoculations of antidotal substance. It is greatly to be regretted that owing to general ignorance of this fundamental principle of M. Pasteur's method the many and violent discussions upon his treatment have been rendered absolutely useless. We trust, therefore, we may be excused for having dwelt thus at length upon it.

As soon as M. Pasteur had demonstrated to the French Academy that he could secure the *protection* of dogs by injecting them in the manner above described, he was induced to attempt the *prevention* of the disease in man by similar injections after the bite. This attempt soon developed into a regular practice, from the numbers of patients who flocked to Paris seeking treatment in consequence of the unhappy prevalence of rabies in those European countries which had neglected to provide for its easy extermination by suitable legislation.

From the Report of the Committee commissioned by the Local Government Board to inquire into M. Pasteur's treatment, it would appear, as might have been expected, that a comparatively large proportion of M. Pasteur's patients were bitten by dogs which were not rabid ; but it is also evident from the same Report that when deductions have been made for these cases the death-rate among the remainder was far lower than even the lowest estimate ever formed of the mortality from hydrophobia among persons bitten by reputedly rabid dogs.

While this gratifying result was accomplished, it was at the same time evident that the method was by no means perfected, and the lamented death of Lord Doneraile (from the bite of a tame fox which had been infected by a dog), affords an illustration of this, for the deceased nobleman was subjected to treatment within a few days after receiving the virus. But while the inoculations did not prevent a fatal issue, there seems good reason to believe that they notably modified the distressing features of the malady, for in a brief account before us it is stated that the inability to swallow fluids only appeared twenty-four hours before death, and there was at no time spasm produced by swallowing moist solid food. The same gratifying modification appears to have also been present in another case that recently was observed in St. George's Hospital. Should this modification prove to be general, M. Pasteur will have deprived the malady of its worst tortures.

Before leaving the consideration of this part of the subject it is to be noted that the virulent opposition with which M. Pasteur's efforts in the cause of humanity were met went to the length of charging him with having actually caused the death of some of his patients by his inoculations. This charge, though not supported by any exact evidence whatever, was also inquired into by the above-mentioned Committee appointed by the Local Government Board. Indeed, they had a special opportunity of doing so, for one of the laboratory servants of Mr. Horsley (who carried out the experiments for the Committee), being bitten most severely by a rabid cat, died six weeks later (the usual incubation period of the disease) with the paralytic form of hydrophobia rather than the excitable form. Rabbits inoculated from the spinal cord of this man died with the shortest possible period of incubation. As the Committee point out, this would seem to have lent colour to the idea that the inoculations themselves were fatally virulent, had not similar instances of short incubation periods been occasionally observed to follow inoculation from similarly rabid animals. Stress was also laid upon the mode in which the man was inoculated—namely, by what M. Pasteur called the intensive treatment, and which he adopted in cases of very severe injury. But the whole question was dismissed by M. Pasteur altering this mode of treatment in order that there should not even be the semblance of the possibility of such an accident.

It seems to us that, in England at any rate, there is quite another view to be taken of this question of rabies and its scientific prevention—in fact, that its complete extermination should be ensured in preference to efforts to treat it after it has attacked anyone.

The data upon which legislation should be based are now fortunately at hand. The House of Lords recently appointed a Select Committee, under the chairmanship of Lord Cranbrook, the President of the Privy Council, to inquire into the whole question of the social bearing of the disease, and the means which have been adopted to get rid of it in foreign countries. The Report of that Committee is published, and we have been permitted in addition to inspect the evidence laid before it. This evidence is a most instructive comment upon the manner in which the facts of modern science are sometimes treated as being of only equal value with the most absurd misstatements dictated by charlatanism and abandoned self-interest.

As a whole, however, the Report is one with which we have good reason to be satisfied in many ways, for it recognizes the great value of the simplest means of preventing the spread of the disease, viz. the muzzle.

Those of us who remember the senseless anti-vivisectionist opposition which met the police edict enforcing this salutary measure in London, will not be surprised of course to find in the evidence before the Committee the same thing repeated, but, as was inevitably the case, deprived this time of all its deceptive influence.

For the experience of the working of the muzzle in London, where it brought the number of deaths from hydrophobia down from 27 in 1885 to 0 in the last quarter of 1886, and indeed we believe we may also say the first six months of 1887 ; the experience of its working in Nottingham, where the cases of rabies varied directly in number according to the way in which the muzzle regulations were enforced ; of Prussia, where the disease is almost extinct, being one-fiftieth part of that in Great Britain ; of Scandinavia, where it is absolutely extinct—all disproved the baseless theories and assertions of those who, under the guise of pseudo-zoophilism, endeavour to perpetuate in man and the lower animals the torments of this horrible disease. As will doubtless have been already surmised, the whole of this factious opposition to the above-mentioned beneficent legislation came from the small clique of anti-vivisectionists who were unhappily represented on the Committee itself in two of its members, viz. Lords Mount-Temple and Onslow.

The Lords' recommendation of the muzzle, however, is marred by one defect, and that a very serious one.

Here is a disease, a zymotic disease, the virus of which, as we have just seen, is most difficult of isolation, and evidently easily destroyed by ordinary conditions when it has left living tissues; a disease, too, which is fortunately infrequent compared to many others, and again fortunately one which would become extinct if not kept in existence by transmission from dog to dog; a disease, in short, which needs nothing but the firm administration of the most ordinary rules of preventive medicine to be destroyed utterly,—and yet, in deference to the professional agitation before mentioned, the Lords' Committee only recommend the use of the muzzle when the disease is "prevalent." If this means, and it is capable of being interpreted in two ways, that the Lords' Committee think the muzzle should be applied only when the disease is epidemic, nothing more regrettable can be imagined. To the scientific mind it seems almost incredible that a legislative body should hesitate to grasp the opportunity, the easiest ever offered, of eradicating a disease so painful and utterly incurable when once the symptoms have declared themselves; but here unfortunately is the example we referred to above of scientific fact overridden by vulgar prejudice. For this disease, acknowledged by all who have studied it to be transmitted solely by inoculation of one animal by another, is endemic in Great Britain, is paramount in the manufacturing districts and great cities; and yet the Lords' Report, instead of recommending the universal application of the muzzle, which would abolish the evil from these its centres, is content to leave it to the local authority—Heaven save the mark!—to apply the remedy when the disease has already made sufficient havoc (!) as to call for its suppression. It is sad, too, to see that this view, which we must call narrow, runs through the whole Report, but it is gratifying to find that that Committee, at any rate, fully appreciates the high worth of M. Pasteur's invaluable test of the disease.

In conclusion the Lords say that, should M. Pasteur's method of treating the disease be found of value, provision should be made for its introduction into England. While heartily concurring in this recommendation, we cannot but feel grieved that the necessity for it in England should be permitted to exist; for in this country, like Scandinavia, the introduction of the disease can be prevented; so that if proper measures were taken England would enjoy the same complete immunity from it that Sweden does at the present day. With reference to the adoption of M. Pasteur's mode of treatment into this country, a most fundamental difficulty arises at the outset, viz. that we have no public laboratory where investigations of this kind could be carried on for the nation, and that therefore an institution of the kind would have to be established for this and kindred subjects of inquiry. At the present time there is, unfortunately, little hope that this want—which we have before so frequently pointed out is nothing short of a national disgrace—will be adequately met; and, as a matter of fact, questions of this sort are usually decided at the Brown Institution, the nation being thus lamentably dependent upon the assistance of a private charity.

Oddly enough this necessity has just been provided for in France by the institution of a *quasi*-private laboratory—the Pasteur Institute. We are, however, strongly of the opinion that questions of this kind are of an Imperial character, and as such should be dealt with by the central Government in a properly-fitted institution.

Hydrophobia from time immemorial has been the most dreaded of all diseases, and justly; but no doubt this dread has been intensified by ignorance of its causation, an ignorance which, having existed for more than 2000 years, has just been dissipated to an enormous extent by the scientific labours of M. Pasteur. This advance is, of course, a source of vexation to the misanthropic anti-vivisectionists, who are shamefully endeavouring to bolster up the exploded theory of spontaneous generation in order to hamper the efforts of preventive medicine to stamp out the disease, regardless also of the evidence from countries where it has been so rooted out, and where, owing to its importation being prevented, it has never appeared again.

And while we have made this great step forward in our knowledge of the nature and etiology of the disease, we have at the same time learnt, thanks again to M. Pasteur, how to protect animals from its ravages, how to prove or disprove its existence in the absence of clinical or anatomical evidence, and, although this is still *sub judice*, apparently how its fatal effects may be warded off in the human being, and, if not successfully prevented, possibly ameliorated.

Finally, a most satisfactory outcome of this increase in our scientific knowledge is the revelation to us that by the adoption of certain legislative means we may destroy the evil once and for all.

POPULAR BOOKS ON BIRDS.

Ocean Birds. By J. F. Green. With a Preface by A. G. Guillemard, and a Treatise on Skinning Birds, by F. H. H. Guillemard, M.D. With Illustrations by Frances E. Green. 4to, pp. viii.-93. (London: R. H. Porter, 1887.)

Bird Life in England. By Edwin Lester Arnold. 8vo. (London: Chatto and Windus, 1887.)

TWO books deserving the above title are before us. It is well known that some of the most interesting works on ornithology have been written by men who do not profess to be scientific naturalists, but who exhibit an intelligent acquaintance with their subject and also possess a faculty of description the want of which adds so much to the dullness and heaviness of style with many more ambitious writers. Anyone who has made an ocean voyage knows full well that the hours are often apt to hang heavy on the hands of the passengers; and if this is true on board a steamer, it is much more true in the case of a sailing-vessel. Mr. Green therefore has compiled a volume which aims at giving assistance to voyagers in the southern oceans, providing short descriptions of the species of sea-birds most commonly met with; and as the author has travelled much by sea himself, it may be taken for granted that he knows the wants of an ornithological inquirer on board a vessel, and has done his best to supply the information. A "Glossary of Terms," and a chapter on the preparation of bird-skins, have been furnished by the author's friend Dr. Guillemard, whose excellent account of the voyage of the *Marchesa* is one of